



Unemployment Insurance and Unemployment Spells

Bruce D. Meyer

Econometrica, Vol. 58, No. 4. (Jul., 1990), pp. 757-782.

Stable URL:

<http://links.jstor.org/sici?sici=0012-9682%28199007%2958%3A4%3C757%3AUIAUS%3E2.0.CO%3B2-0>

Econometrica is currently published by The Econometric Society.

Your use of the JSTOR archive indicates your acceptance of JSTOR's Terms and Conditions of Use, available at <http://www.jstor.org/about/terms.html>. JSTOR's Terms and Conditions of Use provides, in part, that unless you have obtained prior permission, you may not download an entire issue of a journal or multiple copies of articles, and you may use content in the JSTOR archive only for your personal, non-commercial use.

Please contact the publisher regarding any further use of this work. Publisher contact information may be obtained at <http://www.jstor.org/journals/econosoc.html>.

Each copy of any part of a JSTOR transmission must contain the same copyright notice that appears on the screen or printed page of such transmission.

The JSTOR Archive is a trusted digital repository providing for long-term preservation and access to leading academic journals and scholarly literature from around the world. The Archive is supported by libraries, scholarly societies, publishers, and foundations. It is an initiative of JSTOR, a not-for-profit organization with a mission to help the scholarly community take advantage of advances in technology. For more information regarding JSTOR, please contact support@jstor.org.

UNEMPLOYMENT INSURANCE AND UNEMPLOYMENT SPELLS¹

BY BRUCE D. MEYER

This paper tests the effects of the level and length of unemployment insurance (UI) benefits on unemployment durations. The paper particularly studies individual behavior during the weeks just prior to when benefits lapse. Higher UI benefits are found to have a strong negative effect on the probability of leaving unemployment. However, the probability of leaving unemployment rises dramatically just prior to when benefits lapse. When the length of benefits is extended, the probability of a spell ending is high in the week benefits were previously expected to lapse. Individual data are used with accurate information on spell durations, and the level and length of benefits. Semiparametric estimation techniques are used and compared to alternative approaches. The semiparametric approach yields more plausible estimates and provides useful diagnostics.

KEYWORDS: Unemployment spells, unemployment insurance, disincentive effects, hazard models, semiparametric duration analysis.

1. INTRODUCTION

THIS PAPER EXAMINES the effects of the level and potential duration of unemployment insurance (UI) benefits on unemployment durations.² The paper particularly studies individual behavior during the weeks just prior to when benefits lapse. Higher UI benefits are found to have a strong negative effect on the probability of leaving unemployment. However, the probability of leaving unemployment rises dramatically just prior to when benefits lapse. When the length of benefits is extended, the probability of a spell ending is high in the week benefits were previously expected to lapse.

The study uses individual data with accurate information on spell durations, and the level and length of benefits. Much variation in the potential length of UI benefits is available in the eight-state administrative data used in the study. Only about one-half of the individuals have potential durations of 26 or 39 weeks. The many different values of potential duration allow the effect of approaching benefit exhaustion to be separately identified from the time pattern of the exit rate from unemployment.

Semiparametric estimation techniques are used and compared to alternative approaches. Following Prentice and Gloeckler (1978), the approach nonparametrically estimates the effect of time on the exit rate. Meyer (1986) and Han and Hausman (1986) extensively analyze models of this type. The semiparametric estimator yields more plausible coefficients than those obtained from com-

¹ I would like to thank Robert Moffitt for providing the data and answering numerous questions. I have benefited from comments by Hank Farber, Jerry Hausman, Larry Katz, Dale Mortensen, Glenn Sueyoshi, Paula Worthington, two anonymous referees, and seminar participants at Chicago, Columbia, MIT, Minnesota, Northwestern, and the NBER Labor Studies Group. The Sloan Foundation, the Industrial Relations Section at Princeton University, and the NSF through Grant SES-8821721 provided financial support.

² Excellent surveys of the unemployment insurance literature can be found in Hamermesh (1977), Welch (1977), and Danziger, Haveman, and Plotnick (1981).

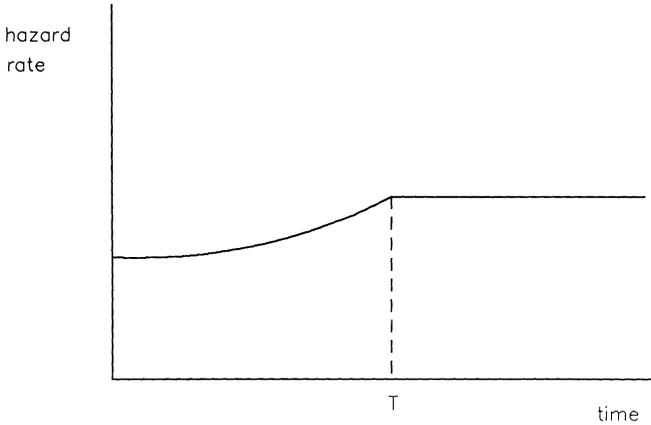


FIGURE 1.

monly used Weibull models. The approach also provides useful diagnostics as the estimated baseline hazard parameters are indicators of possible omitted variables.

2. THEORY

Unemployment spells with finite duration UI have been modeled in several ways. Mortensen (1977) uses a dynamic search model. In his model, individuals maximize the present value of expected utility, where utility is a function of income and leisure. There is no saving in the model; individuals consume their income. A stationary known wage offer distribution is assumed and the arrival rate of job offers is constant over time for a given search intensity. Mortensen's escape rate or hazard is proportional to $s[1 - F(w)]$, where s is the search intensity, w is the reservation wage, and F is the cumulative distribution of wage offers. The hazard rises with search intensity because the arrival rate of job offers increases. The hazard also rises as the reservation wage declines since the probability of an offer being acceptable rises. As one gets closer to when benefits lapse, s is shown to increase, while w decreases as exhaustion approaches.³ Both derivatives imply that the hazard rises until the point of UI exhaustion. After exhaustion, the environment facing an unemployed individual does not change, implying a constant hazard. The time pattern of the hazard is shown in Figure 1, where T is the length of UI benefits.⁴

³ The reservation wage is determined by the equality between the value of being employed at one's reservation wage and being unemployed (equation 9(a) in Mortensen (1977)). The reservation wage must decline as exhaustion approaches since the value of being unemployed drops. Search intensity is determined by the equality of the marginal cost of search (lost leisure) and the marginal benefit of search (equation 9(b) in Mortensen (1977)). Since the marginal benefit of search increases when the value of unemployment declines, search intensity increases.

⁴ The figure is drawn assuming the marginal utility of leisure is independent of income. If leisure and income are complements (substitutes), the hazard is discontinuous at T and higher (lower) after T than shown.

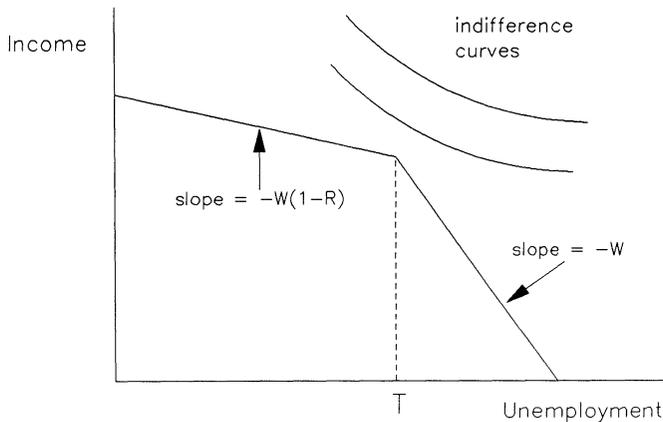


FIGURE 2.

Moffitt and Nicholson (1982) use a version of the static labor-leisure model of individual choice. In this model people have preferences over two goods, income and unemployment. Unemployment has utility because of its leisure value. The new job wage is fixed and a job can be found at any time. At the time of job loss, an individual chooses income and weeks of unemployment subject to a budget constraint. The budget constraint has a convex kink at the week of benefit exhaustion because unemployment ceases to be subsidized. Figure 2 shows the budget constraint. W is the wage, R is the fraction of the wage replaced by UI benefits, and T is the length of benefits. An indifference curve through the kink point can have an implied marginal rate of substitution equal to any value between the slopes of the two budget constraint segments. Thus, with a continuous distribution of preferences, many people will maximize their utility by returning to work the week benefits lapse. Moffitt and Nicholson then argue informally that the random nature of job finding will cause a clustering of observations around the exhaustion point.

The two models make very different assumptions but have similar predictions. In the Mortensen model the individual is uncertain when a job will be found and what the wage will be. One remains unemployed until a sufficiently high paying job is found. In the Moffitt and Nicholson model one can find a job anytime at a fixed wage. Their model emphasizes the leisure value that a period of not working may have if one optimizes over a long period of time such as a year. This explanation is the most plausible when there is a significant demand for home production or it is difficult to take a vacation once a new job has begun.⁵ Both models predict a rising hazard as the UI exhaustion point approaches. Because of the stationarity assumptions, Mortensen's model implies a monotonic increase in the hazard as exhaustion approaches. The Moffitt and

⁵ Implicit in this discussion is the assumption that the search requirement for UI receipt can be satisfied at low cost.

Nicholson model is less precise but a rise in the hazard around the exhaustion point is implied.

There are several aspects of UI unemployment spells that are not captured by these models. The length of UI benefits often changes in the course of an unemployment spell. In the sample studied below about 47 percent of UI recipients experienced a change in their length of benefits. Second, individuals may make arrangements to return to a job several weeks before they actually do. Third, recall is quantitatively more important than new job finding for most UI recipients. Using data from two states, Corson and Nicholson (1984) and Katz and Meyer (1988) find that about 60 percent of spells end in recall. Katz (1985) provides a clear discussion of search with recall and infinite length unemployment insurance. However, recall dates likely depend on individual or typical UI benefit lengths. If workers are bound to firms by implicit contracts, moving costs, specific human capital, or other reasons, firms have an incentive to base recall decisions on the length of UI benefits.

Mortensen (1987) provides a model with some of these characteristics. He analyzes a model of efficient job separations where firms and individuals maximize their joint wealth. Individuals are subject to stochastic changes in their productivity which cause layoffs or recalls. Unemployment benefits have a limited duration. The discrete change in the flow value of being unemployed when benefits are exhausted yields the prediction that many firms may recall laid-off workers around the benefit exhaustion point. Incorporating parameter estimates from several empirical studies, Mortensen further predicts that most of the rise in the hazard will occur in the last few weeks before benefits expire.

3. DATA

The data are from Continuous Wage and Benefit History (CWBH) UI administrative records previously used by Moffitt (1985a, 1985b). Males from twelve states during the period 1978–1983 are examined.⁶ The advantage of CWBH data is accurate information on weeks of UI receipt, pre-unemployment earnings, the level of UI benefits, and the potential duration of benefits over time. The importance of accurate data is highlighted by the large degree of measurement error that has been found in the weeks unemployed variable in some household surveys.⁷ Additionally, the UI parameters, benefit level and duration, are often missing from other data sources. The CWBH data provide accurate information on these key variables.

⁶ The twelve states are Georgia, Idaho, Louisiana, Missouri, New Mexico, New York, North Carolina, Pennsylvania, South Carolina, Vermont, Washington, and Wisconsin. Nevada was included in the original data set used by Moffitt, but was excluded here because of missing data mentioned below.

⁷ Poterba and Summers (1984) and Sider (1984) examine the Current Population Survey. Using data matched across two consecutive months, Poterba and Summers check if the reported length of continuing unemployment spells increases by four to five weeks in the course of a month. Allowing for a three-week margin of error they find that only 57 percent of responses are consistent. Sider studies the distribution of reported incomplete unemployment spells. He finds a pronounced tendency to report round numbers such as 26 and 39 weeks. This clustering cannot be due to UI since these are incomplete spell lengths.

TABLE I
DESCRIPTIVE STATISTICS^a

Variable	Minimum	Maximum	Mean	Standard deviation
Age	17.00	80.00	36.42	12.07
Number of dependents	0.00	6.00	1.43	1.45
1 = Married, spouse present	0.00	1.00	0.70	0.46
1 = White	0.00	1.00	0.80	0.40
Years of schooling	0.00	17.00	11.61	2.55
Log UI benefit ^b	2.70	5.08	4.60	.32
Log Pre-UI income after taxes ^b	2.91	6.09	5.05	.42
UI replacement rate	0.19	0.99	0.66	0.16
Initial length of benefits ^c	8.00	55.00	34.26	8.80
State unemployment rate	4.70	14.81	8.70	2.08
Weeks benefits received	1.00	39.00	13.05	10.35

^a $N = 3365$.

^b Benefits and income are in 1977 dollars.

^c Initial length of benefits is the number of weeks of benefits an individual is entitled to when his spell begins.

The disadvantage of the data is that only information on weeks compensated by the UI system is available. Individuals are censored when their benefits lapse, so behavior beyond the exhaustion point cannot be examined. Also, an individual may not receive UI continuously; weeks may be skipped during which benefits are not received.⁸ Weeks of benefit receipt may be more useful than true unemployment spells when unemployment is briefly interrupted but followed by more unemployment. Weeks of benefit receipt may do a better job of grouping together periods of similar behavior.

The data set originally analyzed by Moffitt contains 4,628 observations. Two exclusions⁹ leave 3,365 observations which are analyzed. The sample is composed of two parts of roughly equal size. The first part is a random sample, while the second oversamples from states and time periods that are more likely to have unchanging benefit lengths. Descriptive statistics for the sample are given in Table I. The mean pre-UI weekly income is 169.5 after taxes¹⁰ (in 1977 dollars). The mean weekly benefit is 104.2 dollars, and .70 is the mean UI replacement ratio (benefits divided by after-tax income). The mean beginning of spell state unemployment rate is fairly high during this period at 8.7 percent. The average number of weeks of benefits received is just over 13. This mean includes 985 right censored spells; 201 of these spells were censored at the exhaustion of benefits.

A more complete illustration of the pattern of weeks of UI receipt and censoring can be seen in Table II and Figure 3. Table II gives the empirical

⁸ The sample is restricted to those whose gaps between periods of benefit receipt are cumulatively less than ten weeks. See Moffitt (1985a, 1985b) for a more detailed discussion.

⁹ 1,227 observations have missing data on age, schooling, dependents, or marital status. 36 observations have negative values for time until benefits lapse.

¹⁰ The marginal tax rate was calculated by Walter Corson of Mathematica Policy Research. The calculations use family income and account for state and federal income taxes and Social Security payroll taxes.

TABLE II
 FAILURES, CENSORINGS, AND THE KAPLAN-MEIER EMPIRICAL HAZARD^a

Week t	Risk set R_t	Failures D_t	Censorings C_t	Hazard H_t	Standard error
1	3365	277	0	.08232	.0047
2	3062	203	26	.06630	.0045
3	2832	159	27	.05614	.0043
4	2657	161	16	.06059	.0046
5	2458	123	38	.05004	.0044
6	2271	112	64	.04932	.0045
7	2112	88	47	.04167	.0043
8	1984	82	40	.04133	.0045
9	1850	86	52	.04649	.0049
10	1722	63	42	.03659	.0045
11	1621	68	38	.04195	.0050
12	1520	91	33	.05987	.0061
13	1402	71	27	.05064	.0059
14	1300	58	31	.04462	.0057
15	1210	55	32	.04545	.0060
16	1134	46	21	.04056	.0059
17	1077	60	11	.05571	.0070
18	999	58	18	.05806	.0074
19	936	44	5	.04701	.0069
20	880	41	12	.04659	.0071
21	829	49	10	.05911	.0082
22	773	45	7	.05821	.0084
23	721	44	7	.06103	.0089
24	662	34	15	.05136	.0086
25	610	48	18	.07869	.0109
26	430	45	132	.10465	.0148
27	378	26	7	.06878	.0130
28	317	30	35	.09464	.0164
29	279	21	8	.07527	.0158
30	245	13	13	.05306	.0143
31	226	9	6	.03982	.0130
32	212	17	5	.08019	.0187
33	190	5	5	.02632	.0116
34	178	8	7	.04494	.0155
35	165	13	5	.07879	.0210
36	121	12	31	.09917	.0272
37	105	6	4	.05714	.0227
38	91	9	8	.09890	.0313

^a 2380 failures were observed, and 985 censorings. 201 of the censorings occurred at exhaustion of benefits.

hazard for the data. The empirical hazard is the fraction of spells ongoing at the start of a week which end during the week.¹¹ In Figure 3 there are several periods when the empirical hazard is noticeably higher than surrounding periods. The high hazard in the first several weeks is probably caused by the high

¹¹ Formally, the empirical hazard for week t , H_t , is the number of spells which end during the week, D_t , divided by the size of the risk set at the beginning of the week. The size of the risk set at the beginning of week t , R_t , is just the number of people whose spells have not ended or been censored at the beginning of week t . Algebraically, $H_t = D_t/R_t$. C_t is the number of observations which are censored at the beginning of week t . $C_t = R_{t-1} - D_{t-1} - R_t$.

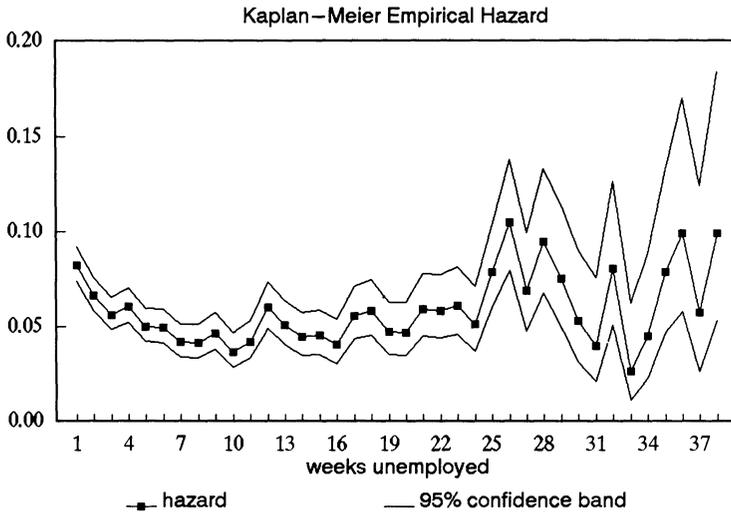


FIGURE 3.—Kaplan–Meier empirical hazard.

frequency of recalls in the early weeks of unemployment.¹² The hazard is higher between 25 and 29 weeks and then again between 35 and 38 weeks. These jumps are probably caused by UI exhaustion. An examination of Table III provides some evidence on this point.

Table III reports the distribution of two measures of the length of UI benefits. Initial length is the number of weeks of benefits an individual is entitled to when his spell begins. Since benefit lengths often change in the course of an individual's spell, the maximum length is also reported. Almost half of the initial lengths are not 26 or 39, and more than half of maximum lengths are not equal to these common values. The rises in the hazard are roughly coincident with weeks when benefits commonly lapse. However the timing of some of the peaks in the hazard is inconsistent with the earlier theories. The peak at 26 for example, cannot be caused by benefits running out after 26 weeks. Such individuals would be censored at 26 so we would not observe the end of their spells. This is discussed in more detail later.

There are several causes of the variability in benefit lengths seen in Table III. First, there is variability across states in the length of regular benefits provided. During the sample period, Louisiana typically provided 28 weeks, Pennsylvania provided 30 weeks, while most other states provided 26 weeks of benefits. Second, benefits were extended during periods of high unemployment under several federal programs. The Extended Benefits program extended benefits 50 percent beyond state durations, up to a maximum of 39 weeks, whenever the insured unemployment rate was above a trigger level. In 1981, the system changed from a state or federal trigger to a higher, state only trigger. Two other programs provided supplemental benefits. At the beginning of the sample

¹² See Corson and Nicholson (1983) and Katz (1985, 1986).

TABLE III
 FREQUENCY TABLE FOR INITIAL AND MAXIMUM LENGTH OF BENEFITS

Number of weeks	Initial length ^a	Maximum length ^b
1	0	0
2	0	0
3	0	0
4	0	0
5	0	0
6	0	0
7	0	0
8	1	1
9	0	0
10	0	0
11	0	0
12	0	0
13	0	0
14	3	3
15	2	2
16	6	3
17	9	9
19	6	5
20	24	19
21	27	17
22	21	17
23	28	21
24	42	29
25	27	18
26	988	645
27	10	8
28	208	154
29	8	5
30	115	44
31	3	6
32	42	46
33	17	18
34	9	5
35	15	20
36	230	258
37	2	1
38	49	36
39	890	919
40	145	138
41	2	1
42	12	3
43	4	7
44	22	7
45	3	1
46	4	6
47	3	9
48	3	15
49	122	222
50	53	67
51	0	0
52	3	94
53	124	296
54	0	0
55	76	186

^a Initial length is the number of weeks of benefits an individual is entitled to when his spell begins.

^b Maximum length is the maximum number of weeks a person becomes entitled to in the course of his spell. Maximum length exceeds initial length whenever benefits are extended.

TABLE IV
TIME UNTIL BENEFITS LAPSE EMPIRICAL HAZARD^a

Weeks left	Risk set	Failures	Censorings	Hazard	Standard error
1	303	50	1	.165017	.0213
2	336	24	3	.071429	.0140
3	390	38	1	.097436	.0150
4	430	30	2	.069767	.0123
5	466	31	1	.066524	.0115
6	505	25	1	.049505	.0097
7	543	28	1	.051565	.0095
8	578	28	0	.048443	.0089
9	617	28	5	.045381	.0084
10	673	38	3	.056464	.0089
11	755	52	5	.068874	.0092
12	825	44	5	.053333	.0078
13	909	54	16	.059406	.0078
14	1011	59	5	.058358	.0074
15	1084	49	6	.045203	.0063
16	1132	40	6	.035336	.0055
17	1217	63	7	.051767	.0064
18	1300	59	3	.045385	.0058
19	1367	68	8	.049744	.0059
20	1435	72	7	.050174	.0058
21	1496	69	6	.046123	.0054
22	1577	91	9	.057705	.0059
23	1654	82	14	.049577	.0053
24	1774	121	15	.068207	.0060
25	1954	138	13	.070624	.0058
26	1030	52	22	.050485	.0068
27	1101	66	24	.059946	.0072
28	956	48	27	.050209	.0071
29	1036	42	21	.040541	.0061
30	986	50	27	.050710	.0070
31	1040	55	22	.052885	.0069
32	1071	67	14	.062558	.0074
33	1124	58	21	.051601	.0066
34	1210	74	20	.061157	.0069
35	1281	66	19	.051522	.0062
36	1110	71	6	.063964	.0073
37	1193	78	23	.065381	.0072
38	1231	85	21	.069050	.0072
39	370	15	19	.040541	.0103
40	253	11	19	.043478	.0128
41	268	11	14	.041045	.0121
42	265	9	25	.033962	.0111
43	303	18	20	.059406	.0136
44	300	12	26	.040000	.0113
45	312	13	24	.041667	.0113
46	325	9	23	.027692	.0091
47	355	21	30	.059155	.0125
48	375	20	25	.053333	.0116
49	275	15	23	.054545	.0137
50	239	9	13	.037657	.0123
51	228	11	12	.048246	.0142
52	202	8	11	.039604	.0137
53	78	2	8	.025641	.0179
54	76	3	0	.039474	.0223

^a This empirical hazard differs from the Kaplan-Meier hazard with the time axis reversed for two reasons. First, people begin with different lengths of benefits. Second, the time until benefits lapse does not always decrease by one each week, since the length of benefits often changes. Thus, the accounting identity given in footnote 8 will not hold and a person may appear more than once in the risk set for a given week.

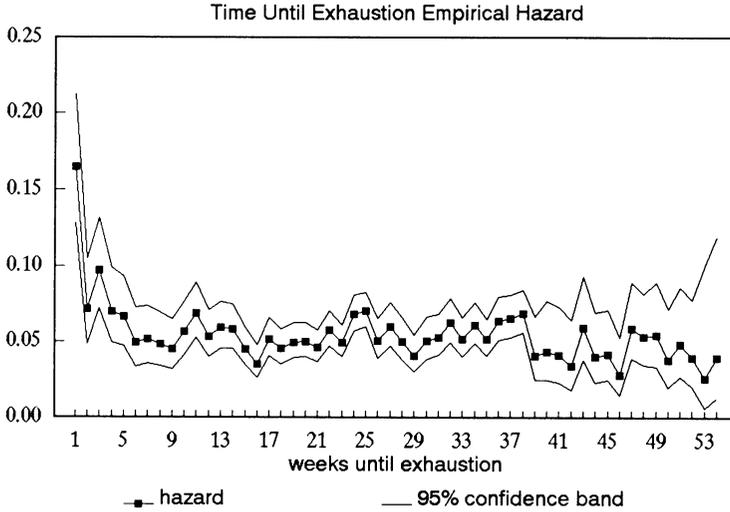


FIGURE 4.—Time until exhaustion empirical hazard.

period, the Federal Supplemental Benefits program provided up to a total of 65 weeks of benefits. Beginning in the Fall of 1982 the Federal Supplementary Compensation program provided up to 62 weeks of benefits. Lastly, within a state at a point in time the length of benefits may depend on an individual's work history. The distribution of benefit lengths reported in Table 3 reflects all of these factors.

I initially examine the effects of finite length UI benefits nonparametrically and without explanatory variables. Table IV gives an empirical hazard analogous to the Kaplan-Meier estimator. Figure 4 displays a graph of the hazard. The time axis is time until benefits lapse rather than time since a spell began. There is a noticeable rise in the hazard about five weeks before benefits lapse. The hazard also jumps dramatically the week before benefits end. For the other weeks there is no discernable trend, except a somewhat lower hazard when exhaustion is more than nine months away.

4. DURATION MODELS

Several aspects of Table IV point to the need for more sophisticated modeling. The hazard is high at 24, 25, 36, 37, and 38 weeks before exhaustion. This is due to the large number of people with initial durations of 26 and 39 weeks, and the higher baseline hazard in the first few weeks of unemployment as seen in Figure 3. Additionally, the Kaplan-Meier hazard assumes that the sample is homogeneous, i.e., that there is no heterogeneity which depends on either observable or unobservable factors. However, one expects that the characteristics causing a lower hazard will be more concentrated among the remaining individuals as one approaches exhaustion. For example, the remaining observations are likely to be disproportionately high benefit, nonwhite, and older. This

sorting effect may mask a much larger increase in the hazard as exhaustion approaches. These problems are potentially solved by using a duration model. If the effect of time since the beginning of a spell is handled in a flexible manner it should account for the higher hazard just after 26 and 39 weeks until exhaustion. Similarly, one can look at the pure effect of getting closer to exhaustion, holding other explanatory variables constant.

The importance of time dependent covariates and censoring in the data make a duration model especially useful. The theories of unemployment behavior with UI discussed earlier imply that the hazard should increase as exhaustion approaches. It is difficult to allow for this with explanatory variables constant over time. The initial duration of benefits does not reflect future extensions. The maximum duration of benefits is an endogenous variable since benefits have a higher probability of being extended if an unemployment spell lasts longer.¹³ This suggests using a (not necessarily linear) function of time until exhaustion to account for the length of benefits. Regression approaches to this problem are further plagued by the censoring of over a quarter of the spells. The biases from censoring are discussed extensively by Welch (1977) in his critique of UI studies. Greene (1981) and Chung and Goldberger (1984) derive the magnitude of the censoring bias under certain assumptions. Alternatively, one can assume a shape for the distribution of spells and use Tobit type techniques, but estimates are very sensitive to the assumed shape.¹⁴ A duration model can be less parametric about the shape of the distribution and still allow censoring.

The estimation approach used here is an extension of Prentice and Gloeckler (1978) which is discussed extensively in Meyer (1986).¹⁵ The shape of the hazard is nonparametrically estimated. In this respect the approach is similar to the method used successfully by Moffitt (1985a).¹⁶ However, the approach taken here has several advantages. The estimates are parameters of a continuous time hazard model and thus retain an easy interpretation. Second, the probabilities of surviving each period are constrained to lie between 0 and 1. Third, there is a large literature discussing the importance of allowing for unobservable differences across people.¹⁷ These differences are usually called unobserved heterogeneity. It is relatively easy to test for heterogeneity and estimate it parametrically in this model. The distribution of the heterogeneity component can be nonparametrically estimated as well.

¹³ Consider the case where the initial length of benefits is the same for all individuals and assume that the length of benefits does not affect the length of spells. If benefits have a positive probability of being extended each period, the expected value of the maximum duration will be a monotonic function of the dependent variable.

¹⁴ Moffitt (1985b) examines two unemployment spell data sets and finds the estimates to be quite sensitive to the distribution assumed. Alternatively, distributional assumption could be reduced by incorporating time-varying covariates in semiparametric estimators for Tobit models such as Powell (1984).

¹⁵ Also see Han and Hausman (1986) who propose a similar technique.

¹⁶ Green and Shoven (1986) use an approach close to Moffitt's in their examination of mortgage prepayments.

¹⁷ Examples include Lancaster (1979) and Heckman and Singer (1984).

Formally, let T_i be the length of individual i 's unemployment spell. Then the hazard for individual i at time t , $\lambda_i(t)$, is defined by the equation

$$\lim_{h \rightarrow 0^+} \frac{\text{prob}[t+h > T_i \geq t | T_i \geq t]}{h} = \lambda_i(t).$$

The hazard is parameterized here using a proportional hazards form, i.e.

$$\lambda_i(t) = \lambda_0(t) \exp\{z_i(t)' \beta\},$$

where $\lambda_0(t)$ is the baseline hazard at time t , which is unknown, $z_i(t)$ is a vector of time dependent explanatory variables (covariates) for individual i , and β is a vector of parameters which is unknown.

The probability that a spell lasts until time $t + 1$ given that it has lasted until t is easily written as a function of the hazard:

$$\begin{aligned} (1) \quad P[T_i \geq t + 1 | T_i \geq t] &= \exp\left[-\int_t^{t+1} \lambda_i(u) du\right] \\ &= \exp\left[-\exp(z_i(t)' \beta) \cdot \int_t^{t+1} \lambda_0(u) du\right] \end{aligned}$$

given that $z_i(t)$ is constant between t and $t + 1$. Equation (1) can be rewritten as

$$(2) \quad P[T_i \geq t + 1 | T_i \geq t] = \exp[-\exp(z_i(t)' \beta + \gamma(t))]$$

where

$$(3) \quad \gamma(t) = \ln\left\{\int_t^{t+1} \lambda_0(u) du\right\}.$$

The likelihood function for a sample of N individuals can be written as a function of terms such as (2).¹⁸

$$\begin{aligned} (4) \quad l(\gamma, \beta) &= \prod_{i=1}^N \left[1 - \exp\{-\exp[\gamma(k_i) + z_i(k_i)' \beta]^{\delta_i}\} \right. \\ &\quad \left. \times \prod_{t=1}^{k_i-1} \exp\{\exp[\gamma(t) + z_i(t)' \beta]\} \right], \end{aligned}$$

where $\gamma = [\gamma(0)\gamma(1)\dots\gamma(T-1)]$, C_i is the censoring time, $\delta_i = 1$ if $T_i \leq C_i$ and 0 otherwise, and $k_i = \min(\text{int}(T_i), C_i)$. For equation (4) to be the correct likelihood function, censoring must not provide any information about T_i beyond that available in the covariates and censoring must occur only at the beginning or end of intervals. The first term of (4) is equal to 1 except when a spell ends between k_i and $k_i + 1$, i.e. $\delta_i = 1$. The term which includes the product over $t = 1$ to $k_i - 1$ is just the probability that a spell lasts at least until k_i .

¹⁸ See equation (2.6) and the surrounding text in Meyer (1986) for a complete description.

The corresponding log-likelihood function is

$$(5) \quad L(\gamma, \beta) = \sum_{i=1}^N \left[\delta_i \log \left[1 - \exp \left\{ - \exp \left[\gamma(k_i) + z_i(k_i)' \beta \right] \right\} \right] - \sum_{t=1}^{k_i-1} \exp \left[\gamma(t) + z_i(t)' \beta \right] \right].$$

The log-likelihood (5) is formed using differences in the survivor function (one minus the cumulative distribution function) and is the correct likelihood when exact spell lengths are unknown but the intervals during which spells end are known. Alternatively, the model can be interpreted as a convenient discrete model that retains an interpretation as an incompletely observed continuous time hazard model. The typical approach has been to model discrete or grouped failure times using density function terms in a likelihood as if the spells were observed continuously.

To implement this approach, one must censor any ongoing observations at some large spell length T . In principle one could allow T to increase with the sample size. Here, observations lasting more than 39 weeks are censored at 39. Only 2.4 percent of the spells are ongoing at this point. Given the small number of observations lasting more than 39 weeks, one would need to make strong parametric assumptions to make inferences about the pattern of the hazard after 39 weeks. The log-likelihood function (5) is now a function of β and the 38 elements of γ . The likelihood is easily maximized by standard techniques.

A parametric assumption about the baseline hazard can be thought of as putting restrictions on the elements of γ . Often a Weibull form for the baseline hazard has been assumed, despite lack of theoretical support for any particular shape. The approach in this paper allows consistent estimation of β even when the form of the baseline hazard is unknown. The conventional approach, which assumes a parametric form for the baseline hazard, inconsistently estimates β when the assumed baseline hazard is incorrect.

Even when one has specified the baseline hazard correctly, a parametric approach will usually provide only a small increase in efficiency. Meyer (1986) extensively discusses efficiency comparisons of the Prentice and Gloeckler approach and parametric maximum likelihood. The discussion here is heuristic. The Prentice and Gloeckler approach only uses variation in the covariates across observations to estimate β ; the over time variation in the mean of the covariates is absorbed by the baseline hazard parameters. Parametric maximum likelihood uses the part of the variation in the mean of the covariates that remains after projection on the parametric baseline hazard components. For example, in the Weibull case only variation over time that is not captured in a geometric trend would be used to estimate β . In the likely situation that explanatory variables differ more across observations than over time, or when there is a time trend in the mean of the explanatory variables, the semiparametric approach may give up a small amount of efficiency but insures consistency.

The Prentice and Gloeckler approach is similar to Cox's partial likelihood technique (Cox (1972, 1975)). Both estimators make no assumptions about the baseline hazard. Bailey (1984) shows that in a special case partial likelihood is asymptotically equivalent to the Prentice and Gloeckler approach. His equivalence result assumes that exact spell lengths are known and differ for each observation, and assumes that a separate baseline hazard parameter is estimated between each time of failure.¹⁹

If unobserved heterogeneity is present and is assumed to take a multiplicative form the hazard becomes

$$\lambda_i(t) = \theta_i \lambda_0(t) \exp\{z_i(t)' \beta\},$$

where θ_i is a random variable that is assumed to be independent of $z_i(t)$. A log-likelihood is obtained by conditioning on the unobserved θ and then integrating over its distribution. For the Prentice and Gloeckler model one obtains

$$(6) \quad L(\gamma, \beta, \mu) = \sum_{i=1}^N \log \left[\int \exp \left[-\theta \sum_{t=0}^{k_i-1} \exp\{\gamma(t) + z_i(t)' \beta\} \right] d\mu(\theta) \right. \\ \left. - \delta_i \int \exp \left[-\theta \sum_{t=0}^{k_i} \exp\{\gamma(t) + z_i(t)' \beta\} \right] d\mu(\theta) \right].$$

To implement this approach one typically assumes a shape for the distribution $\mu(\theta)$. Estimation without this assumption is discussed below.

A convenient and commonly used distribution for θ is the gamma. The gamma distribution gives a closed form expression for the likelihood, avoiding numerical integration.²⁰ The gamma distribution is used in this paper; other distributions could in principle be used. If θ is distributed gamma with mean one (a normalization) and variance σ^2 , then the log-likelihood becomes

$$(7) \quad L(\gamma, \beta, \sigma^2) = \sum_{i=1}^N \log \left\{ \left[1 + \sigma^2 \cdot \sum_{t=0}^{k_i-1} \exp\{\gamma(t) + z_i(t)' \beta\} \right]^{-\sigma^{-2}} \right. \\ \left. - \delta_i \left[1 + \sigma^2 \cdot \sum_{t=0}^{k_i} \exp\{\gamma(t) + z_i(t)' \beta\} \right]^{-\sigma^{-2}} \right\}.$$

Even if the distribution of θ_i is unknown, γ and β can be consistently estimated using an extension of the Heckman and Singer (1984) approach. Meyer (1986) proves the consistency of this approach by verifying the conditions of Kiefer and

¹⁹ In an empirical study of occupational mobility, McCall (1990) estimates several models using partial likelihood and the Prentice and Gloeckler approach. The coefficient estimates and standard errors are similar for the two approaches.

²⁰ The inverse Gaussian distribution also has this property. See Hougaard (1984).

Wolfowitz (1956). The maximum likelihood estimate of the distribution of θ will be a discrete distribution with a finite number of mass points. In practice, I have found the computation of the discrete distribution difficult. It is plausible that much of the parameter instability found by Heckman and Singer is due to their assumption of a Weibull baseline hazard. When the baseline hazard is nonparametrically estimated, the choice of heterogeneity distribution may be unimportant. In the sections below, estimates without heterogeneity and with gamma distributed heterogeneity are reported and compared.

5. RESULTS

The effects of unemployment insurance are measured using functions of the benefit level and the time until benefits lapse. The log of the weekly UI benefit and the log of pre-UI earnings are included in most specifications. Similar results are obtained when the level of benefits and income are used and when the UI replacement ratio is tried. High benefits are expected to decrease the hazard because they lower the opportunity cost of search and leisure. All else equal, high previous earnings are expected to raise the hazard since the cost of unemployment in terms of lost wages is higher and because high earnings are likely correlated with a better distribution of wage offers.

The other UI variables are *UI 1* to *UI 41–54* which form a spline in time until benefit exhaustion. The coefficient on *UI 2–5* is the additional effect on the hazard of having moved 1 week closer to exhaustion when one is 2–5 weeks away. The coefficient on *UI 1* is the additional effect on the hazard when one moves from 2 to 1 week from exhaustion. Thus, the effect of moving from 6 weeks away to 1 week is 4 times the *UI 2–5* coefficient plus the *UI 1* coefficient. The other UI coefficients have analogous interpretations.

Formally, let τ be the number of weeks until benefits lapse. Then *UI 1* = 1 if $\tau = 1$, and 0 otherwise; *UI 2–5* = $\min(6 - \tau, 4)$ if $\tau \leq 5$, and 0 otherwise; *UI 6–10* = $\min(11 - \tau, 5)$ if $\tau \leq 10$, and 0 otherwise; and similarly for the remaining variables.

The theories discussed in Section 2 predict that the exhaustion spline coefficients should be positive. The prediction depends on the stationarity of the offer arrival function and the wage distribution in Mortensen (1977). If the stationarity assumptions are relaxed, the predictions of the model are indeterminate. However, one might well find that the exhaustion effect dominates in the weeks just before exhaustion, implying positive coefficients for these spline segments. In Moffitt and Nicholson (1982) the prediction for the segments far from exhaustion depends on the distribution of preferences across individuals. Thus, the most robust prediction seems to be positive coefficients on the segments close to exhaustion.

The results are reported in Tables V through VII. The coefficients on the explanatory variables are in Tables V and VI. The simpler specifications are discussed first, then the more sophisticated ones. The weekly benefit and after-tax weekly earnings coefficients have the expected signs and are precisely

TABLE V
HAZARD MODEL ESTIMATES^a

Variable	Specification				
	(1)	(2)	(3)	(4)	(5)
Number of dependents	-.0418 (0.0169)	-.0422 (0.0171)	-.0416 (0.0168)	-.0386 (0.0239)	-.0386 (0.0242)
1 = married, spouse present	.1302 (0.0508)	.1221 (0.0515)	.1315 (0.0507)	.1006 (0.0722)	.1001 (0.0730)
1 = white	.2097 (0.0572)	.2230 (0.0579)	.2171 (0.0568)	.2337 (0.0834)	.2364 (0.0841)
Years of schooling	-.0276 (0.0083)	-.0275 (0.0084)	-.0272 (0.0083)	-.0177 (0.0123)	-.0176 (0.0124)
Log UI benefit level	-.8782 (0.1091)	-.8157 (0.1096)	-.8478 (0.1088)	-.8685 (0.2042)	-.8757 (0.2065)
Log pre-UI after tax wage	.5630 (0.0855)	.5651 (0.0860)	.5530 (0.0848)	.7289 (0.1415)	.7411 (0.1433)
Age 17-24	.2596 (0.0855)	.2613 (0.0865)	.2636 (0.0855)	.2664 (0.1242)	.2670 (0.1256)
Age 25-34	.1545 (0.0750)	.1542 (0.0759)	.1529 (0.0749)	.1080 (0.1066)	.1068 (0.1078)
Age 35-44	.1642 (0.0776)	.1594 (0.0787)	.1621 (0.0774)	.1466 (0.1110)	.1492 (0.1122)
Age 45-54	.0473 (0.0828)	.0417 (0.0837)	.0460 (0.0827)	.0234 (0.1156)	.0239 (0.1169)
State unemployment rate	-.0237 (0.0133)	.0019 (0.0126)	-.0234 (0.0134)	.0967 (0.0216)	.0993 (0.0218)
Exhaustion spline: ^b					
UI 1	.6772 (0.2470)	.6473 (0.1996)	.5977 (0.2479)	.7379 (0.2499)	.6670 (0.2513)
UI 2-5	.1288 (0.0612)	.1468 (0.0519)	.1665 (0.0618)	.1448 (0.0625)	.1847 (0.0634)
UI 6-10	.0054 (0.0317)	.0183 (0.0280)	.0012 (0.0317)	.0054 (0.0334)	.0052 (0.0336)
UI 11-25	-.0052 (0.0068)	.0074 (0.0063)	-.0067 (0.0068)	-.0093 (0.0078)	-.0102 (0.0078)
UI 26-40	-.0018 (0.0064)	.0016 (0.0063)	-.0008 (0.0064)	-.0001 (0.0074)	.0015 (0.0075)
UI 41-54	.0211 (0.0133)	.0264 (0.0133)	.0209 (0.0134)	.0291 (0.0152)	.0289 (0.0152)
Benefits previously expected to lapse ^c			1.4643 (0.1876)		1.6280 (0.2006)
State fixed effects	no	no	no	yes	yes
Nonparametric baseline	yes ^d	no	yes ^d	yes	yes ^d
Heterogeneity variance				.7560 (0.1943)	.7901 (0.1953)
Sample size	3365	3365	3365	3365	3365
Log-likelihood value	-9038.07	-9085.06	-9015.68	-8927.80	-8901.94

^a Standard errors are shown in parentheses.

^b The exhaustion spline variables are defined in the text.

^c If earlier in the spell benefits were expected to lapse in the current week, the variable equals 1; otherwise it equals 0.

^d Baseline hazard parameters are reported in Table VII.

^e The unconstrained estimate of the variance is zero.

TABLE VI
ADDITIONAL HAZARD MODEL ESTIMATES^a

Variable	Specification			
	(6)	(7)	(8)	(9)
Number of dependents	-.0291 (0.0172)	-.0286 (0.0171)	-.0213 (0.0286)	-.0263 (0.0235)
1 = married, spouse present	.0914 (0.0510)	.0972 (0.0507)	.1139 (0.0861)	.0956 (0.0699)
1 = white	.1605 (0.0581)	.1597 (0.0578)	.1339 (0.1080)	.1768 (0.0801)
Years of schooling	-.0118 (0.0085)	-.0124 (0.0085)	-.0085 (0.0150)	-.0107 (0.0129)
Log UI benefit level	-.6021 (0.1390)	-.5993 (0.1386)	-.5326 (0.2420)	-.5683 (0.1848)
Log Pre-UI after tax wage	.5112 (0.0938)	.4926 (0.0943)	.5327 (0.1643)	.2975 (0.1391)
Age 17-24	.2291 (0.0863)	.2355 (0.0859)	.3116 (0.1541)	.0350 (0.1172)
Age 25-34	.1072 (0.0758)	.1110 (0.0754)	.1725 (0.1331)	-.0411 (0.1031)
Age 35-44	.1001 (0.0789)	.1070 (0.0787)	.0180 (0.1367)	.0505 (0.1049)
Age 45-54	.0314 (0.0836)	.0323 (0.0832)	.0354 (0.1447)	-.0674 (0.1152)
State unemployment rate	.0794 (0.0153)	.0588 (0.0166)	.0389 (0.0263)	.3104 (0.0363)
Exhaustion spline: ^b				
UI 1	.6430 (0.2002)	.5772 (0.2490)	.8942 (0.4627)	.4978 (0.3182)
UI 2-5	.1610 (0.0521)	.1669 (0.0621)	.1538 (0.1271)	.0595 (0.0746)
UI 6-10	.0183 (0.0281)	.0051 (0.0320)	.0124 (0.0629)	-.0369 (0.0404)
UI 11-25	.0045 (0.0064)	-.0052 (0.0070)	.0031 (0.0120)	-.0682 (0.0101)
UI 26-40	.0105 (0.0065)	.0063 (0.0068)	-.0089 (0.0099)	-.0525 (0.0112)
UI 41-54	.0253 (0.0136)	.0213 (0.0138)	.0267 (0.0164)	^d
Benefits previously expected to lapse ^c	1.7707 (0.1558)	1.5391 (0.1881)	1.2403 (0.2902)	^d
State fixed effects	yes	yes	yes	yes
Nonparametric baseline	no	yes	yes	yes
Heterogeneity variance	^e		.1829 (0.1936)	^e
Sample size	3365	3365	1844	1521
Log-likelihood value	-8935.95	-8906.79	-3994.54	-4729.65

^a Standard errors are shown in parentheses.

^b The exhaustion spline variables are defined in the text.

^c If earlier in the spell benefits were expected to lapse in the current week, this variable equals 1; otherwise it equals 0.

^d There is insufficient variation in the sample to estimate this coefficient.

^e The unconstrained estimate of the variance is zero.

TABLE VII
 BASELINE HAZARD ESTIMATES FROM SPECIFICATIONS (1), (3), AND (5)

Week	Specification (1)		Specification (3)		Specification (5)	
	Hazard	Standard Error	Hazard	Standard Error	Hazard	Standard Error
1	0.0825	0.0051	0.0826	0.0051	0.0768	0.0051
2	0.0664	0.0047	0.0665	0.0048	0.0673	0.0051
3	0.0563	0.0046	0.0564	0.0046	0.0609	0.0055
4	0.0612	0.0050	0.0613	0.0050	0.0704	0.0068
5	0.0505	0.0047	0.0506	0.0047	0.0617	0.0070
6	0.0499	0.0049	0.0500	0.0049	0.0648	0.0081
7	0.0421	0.0046	0.0422	0.0046	0.0576	0.0080
8	0.0418	0.0048	0.0420	0.0048	0.0597	0.0089
9	0.0473	0.0053	0.0474	0.0053	0.0700	0.0106
10	0.0372	0.0049	0.0373	0.0049	0.0568	0.0098
11	0.0428	0.0055	0.0429	0.0055	0.0674	0.0118
12	0.0619	0.0070	0.0621	0.0070	0.1016	0.0176
13	0.0521	0.0066	0.0523	0.0067	0.0898	0.0172
14	0.0459	0.0064	0.0461	0.0065	0.0822	0.0167
15	0.0469	0.0068	0.0472	0.0068	0.0873	0.0190
16	0.0417	0.0066	0.0418	0.0066	0.0795	0.0182
17	0.0575	0.0081	0.0579	0.0082	0.1142	0.0260
18	0.0602	0.0087	0.0607	0.0088	0.1255	0.0301
19	0.0481	0.0080	0.0483	0.0080	0.1043	0.0273
20	0.0476	0.0082	0.0478	0.0083	0.1065	0.0295
21	0.0596	0.0096	0.0595	0.0096	0.1375	0.0377
22	0.0570	0.0094	0.0561	0.0092	0.1353	0.0384
23	0.0571	0.0099	0.0555	0.0097	0.1390	0.0414
24	0.0462	0.0091	0.0442	0.0087	0.1157	0.0367
25	0.0551	0.0108	0.0537	0.0106	0.1461	0.0482
26	0.1126	0.0189	0.0653	0.0127	0.1876	0.0626
27	0.0653	0.0145	0.0647	0.0144	0.1928	0.0695
28	0.1055	0.0213	0.0933	0.0198	0.2896	0.1090
29	0.0817	0.0193	0.0823	0.0194	0.2813	0.1121
30	0.0589	0.0170	0.0487	0.0139	0.1766	0.0773
31	0.0422	0.0147	0.0425	0.0149	0.1569	0.0776
32	0.0858	0.0228	0.0808	0.0215	0.3137	0.1389
33	0.0269	0.0125	0.0264	0.0123	0.1083	0.0635
34	0.0429	0.0158	0.0413	0.0155	0.1697	0.0905
35	0.0588	0.0177	0.0572	0.0172	0.2455	0.1185
36	0.0916	0.0289	0.0547	0.0169	0.2429	0.1205
37	0.0458	0.0202	0.0423	0.0187	0.1892	0.1165
38	0.0499	0.0186	0.0454	0.0174	0.2022	0.1149

estimated. High wages and low benefits increase the hazard.²¹ Using the coefficient estimates from specification (1), which are typical, a 10 percent increase in benefits is associated with an 8.8 percent decrease in the hazard. The time until exhaustion spline coefficients are jointly highly significant; two of

²¹ A Wald test of the hypothesis that the log benefit and log earnings coefficients are the same size but of opposite sign was performed for specification (5). The test statistic is asymptotically distributed chi-square with one degree of freedom. The statistic is 1.06, so it does not reject the null hypothesis of equality. This finding tends to support the use of the replacement rate in studies of the effect of UI on unemployment spells. The finding is also consistent with a common homogeneity property of search models discussed in Mortensen (1987).

the coefficients are significant individually. The point estimates indicate that moving from 54 to 41 weeks from exhaustion increases the hazard by 32 percent. Between 41 to 6 weeks the hazard is basically flat, but the point estimate is a small decrease in the hazard. From 6 to 2 weeks until exhaustion the hazard rises 67 percent, and 1 week away the hazard rises an additional 97 percent. Cumulatively, the hazard more than triples as one moves from 6 weeks to 1 week until exhaustion.²²

The coefficient on the state unemployment rate has the expected sign and is significantly different from zero. In specification (1), the implied effect of a one percentage point increase in the unemployment rate is a 2.4 percent reduction in the hazard. The hazard falls almost monotonically with age. Those 17–24 have the highest hazard while those 55 and over have the lowest.

The Advantages of Semiparametric Estimation

Nonparametrically estimating the baseline hazard has two advantages. First, the baseline hazard parameters defined in equation (3) can provide useful diagnostics. Second, the approach avoids inconsistent estimation of covariate coefficients due to a misspecified baseline hazard. Table VII illustrates the value of the baseline hazard parameters as a diagnostic tool. Even with the time until exhaustion spline included in the specification, spikes in the hazard remain at 26, 28, 32, and 36 weeks. One explanation for the spikes may be arrangements by individuals to be recalled or begin a new job that are made before benefits run out. If benefits are extended in the intervening period, the result would be a higher hazard after common UI exhaustion points. This would accord with the Moffitt and Nicholson model in which the length of unemployment spells is selected when unemployment begins.

This hypothesis is tested by adding a variable equal to 1 in week t , if earlier in the spell it was expected that benefits would lapse at t .²³ The variable is used in specifications (3), and (5) through (8). The coefficient always has a large

²² Most states limit the total dollar amount of benefits paid to an individual during a benefit year. The limit typically depends on an individual's base period earnings. If this limit on total payments is binding, an individual may receive a smaller payment in his or her last week of eligibility. Some individuals may not bother to pick up this smaller final check. The possibility that these smaller final payments could spuriously generate the rise in the hazard just before exhaustion was examined using an additional CWBH sample. The sample includes 38,472 individuals from 8 states during 1979–1984. The 8 states include 7 of the 12 used in this study. I compared the hazard rate in the week before regular benefits lapse when the benefit payment was its full amount and when it was reduced because of the limit on total dollar benefits. Those who received Extended Benefits or Federal Supplemental Compensation benefits were excluded. The hazard was 21 percent higher (.257 compared to .212) when the last payment was less than the full weekly amount. While these comparisons are somewhat crude since I did not control for individual attributes, they support the hypothesis that a lower benefit amount may cause people not to claim their last week of benefits. Because only a slim majority of the final payments are less than the full amount, this effect could only cause a 12 percent increase in the overall hazard the week before benefits lapse. Thus, only a small part of the rise in the hazard just before exhaustion could be explained by this fact.

²³ It is assumed that changes in UI benefit lengths are not foreseen. To predict changes, an individual would need to predict unemployment rates and congressional actions. As long as the prediction is not perfect, one would expect an effect of the old exhaustion date on the hazard.

asymptotic t statistic. The point estimates imply a four- to five-fold increase in the hazard in the week benefits were expected to lapse. When the benefits expected to lapse variable is included, the spikes at 26, 28, and 36 are no longer present, and the one at 32 declines. This result can be seen by examining the baseline hazard parameters in Table VII.

These estimates provide some support for the hypothesis that early in spells some firms and employees plan when unemployment spells will end.²⁴ An alternative explanation for the result might be that some people eligible for extended benefits do not claim them, despite the simplicity of the procedure and the financial reward.²⁵ Whatever the source of the spikes in the hazard, the baseline hazard parameters reported in Table VII have identified an important feature of the data that would have been missed if a simple parametric baseline hazard had been estimated. Anomalies in the hazard that remain after including covariates suggest variables to add or data problems to examine.

The second advantage of nonparametric estimation of the baseline hazard is consistency of covariate coefficients when the shape of the baseline hazard is unknown. Despite lack of theoretical support for any particular shape, numerous authors have fitted models with a Weibull baseline hazard. Specifications (2) and (6) impose a Weibull baseline, while specifications (1) and (7) estimate the baseline hazard nonparametrically, allowing a comparison of the techniques. Specification (7) is compared to (6) to allow a more fair comparison because the Weibull version of (5) has an unrestricted heterogeneity variance of zero. Likelihood ratio tests of the null hypothesis of a Weibull baseline reject, indicating that the Weibull model is misspecified. The chi-square statistics with 36 degrees of freedom are 93.98 and 58.32 for specifications (2) and (6), respectively.²⁶ Note that the Weibull assumption is a much better approximation when the benefits expected to lapse variable is included.

There is some evidence that the Weibull assumption has biased the coefficients, particularly those on the time-varying covariates. In specification (1) the unemployment rate coefficient is negative and significant, while in the Weibull model of specification (2) it is positive. Two of the coefficients of the UI exhaustion spline also change sign, but they are insignificant using either estimator. The coefficients on time-constant covariates are very close in the two sets of estimates. A Hausman specification test was performed on the time-vary-

²⁴ I originally included separate variables for recent changes in the length of benefits (in the last 8 weeks) and changes that took place anytime during the spell. The coefficient on the within the last 8 weeks variable was always larger but never significantly different from the other coefficient.

²⁵ Another possibility could be coding errors in the potential duration of benefits variable. I cannot check for this possibility using only the recoded data previously analyzed by Moffitt. Creating the potential duration of benefits variable is very complicated because of the numerous and changing provisions of state and federal UI laws. I am currently constructing such a potential duration of benefits variable in a new data set that has the original CWBH variables rather than only recoded data.

²⁶ The critical values at the .05 and .01 level are 51.00 and 58.62, respectively. Specification (2) is rejected at conventional significance levels. Specification (6) is rejected at the .05 level, but just passes at the .01 level.

ing covariates, and the test on these 8 coefficients rejected at the .05 level.²⁷ These results confirm intuitive arguments about the effects of misspecifying the baseline hazard. Since the time pattern of the hazard has been misspecified, coefficients on time-varying covariates, which especially depend on the time pattern of the hazard, are more likely to be biased than other coefficients.

Estimates with State Fixed Effects

Specifications (4) through (9) include state fixed effects. Fixed effects are included because omitted state characteristics may affect both unemployment and the generosity of state UI systems. The omission of fixed effects in this situation would bias estimated UI responses.²⁸ Several authors have found state level variables which are correlated with both unemployment and the generosity of UI programs. Medoff (1979) finds that the layoff subsidy from incomplete UI experience rating is higher for union establishments than nonunion establishments. Unionization also leads to greater use of layoffs independent of the UI subsidy. Adams (1986) finds that the UI subsidy per worker to employers is correlated with the diversifiability of state employment, the skewness of state unemployment rates, and several industrial characteristics. However, it is unclear whether the correlations found by Medoff and Adams reflect the political economy of UI legislation or the effects of UI on employment and layoff patterns.²⁹

A comparison of specifications (3) and (7) overwhelmingly supports the presence of state fixed effects. The likelihood ratio test statistic is 217.78 and is distributed chi-square with 11 degrees of freedom under the null hypothesis of no fixed effects.³⁰ There is some evidence that the endogeneity of state UI laws biases the UI benefit coefficient in the estimates without fixed effects. Specifications (3) and (7) are identical except for the use of fixed effects in (7). In the fixed effects specification the coefficient on benefits drops 29 percent in absolute value to $-.60$. All other fixed effects specifications have similar coefficients in the $-.50$ to $-.60$ range, except specifications (4) and (5). The coefficients in specifications (4) and (5) have a slightly different interpretation because of the large unobserved heterogeneity variance. The inclusion of unobserved heterogeneity tends to increase the absolute value of coefficients, even though ex-

²⁷ The test statistic is 16.95 and is asymptotically distributed chi-square with 8 degrees of freedom. The critical values are 15.51 and 20.09 at the .05 and .01 levels respectively. In order to guarantee that the difference of variance matrices needed for the Hausman test is positive definite, the score functions used to calculate the variance of the inefficient estimates are evaluated at the efficient coefficient estimates.

²⁸ The omitted state characteristics are assumed to be constant over the sample period so that fixed effects estimates are consistent.

²⁹ The latter view is adopted by Deere and Miron (1986) who argue that the distribution of state employment by industry is shaped by UI laws.

³⁰ The critical values at the .05 and .01 level are 19.68 and 24.72, respectively. All else equal, the states with the lowest hazards are Nevada, New Mexico, and Wisconsin. Missouri, North Carolina, and South Carolina have the highest hazards.

pected duration elasticities may not change much.³¹ The exhaustion spline and benefits expected to lapse variables do not appreciably change.

Even after state fixed effects have removed the state mean UI benefit as a source of variation in payments across individuals, there is still a large amount of variation that is not colinear with previous earnings. During the sample period, most of the states have several increases in their maximum UI benefit after inflation has eroded its real level. Most of the increases are in the 5% to 15% percent range and the increases affect the large fraction of recipients for whom the maximum is binding. There are also increases in the minimum benefit, but they are less frequent and affect many fewer people.

The introduction of state fixed effects dramatically changes the state unemployment rate coefficient. The unemployment rate is the monthly CPS state unemployment rate interpolated to give a weekly series. In the specifications with a nonparametric baseline but without state fixed effects (specifications (1) and (3)), the coefficient on unemployment is negative. These specifications estimate the unemployment coefficient using variation in unemployment across states as well as over time. On the other hand, state fixed effects specifications control for the state level of unemployment and only use the time-series variation in unemployment to estimate the coefficient. In the seven specifications with state fixed effects (specifications (4) through (9)), the state unemployment rate coefficient is positive and almost always significantly different from zero.

These results imply that across state and over time variation in unemployment have opposite effects for this time period. States with higher unemployment rates, all else equal, have longer unemployment spells. However, a rise in the unemployment rate over time for a given state is associated with a shortening of unemployment spells in that state. An explanation for this result is that layoffs are countercyclical; in recessions the fraction of unemployment due to layoffs rises.³² Layoff spells also tend to be shorter. Thus, it would not be surprising if the average duration of unemployment spells fell. As mentioned earlier, layoff spells are particularly concentrated among UI recipients.

Allowing for unobserved heterogeneity does not change the conclusions about the effects of UI. Three specifications ((4), (5), and (8)), include gamma distributed unobserved heterogeneity. The coefficients on UI benefits, the time

³¹ The coefficients reported are always the logarithmic derivatives of the hazard with respect to the covariates. Even when these logarithmic derivatives are the same, elasticities of other measures of spell length may change. This rescaling effect is discussed in Lancaster (1979, 1985). In a hazard model with no time-varying regressors, no censoring, and a Weibull baseline hazard, Lancaster (1985) derives the asymptotic bias from omission of heterogeneity. He finds that all coefficients are biased towards zero by the same proportion. However, elasticities with respect to the expected value of the log of duration are unbiased. With censored data and time-varying covariates the appropriate elasticity to report is unclear. A good approach might be to report the elasticities of the probability of a spell lasting a given number of weeks, for a representative path of the covariates.

³² See Feldstein (1975) and Lilien (1980).

until exhaustion spline, and the variable for benefits previously expected to lapse, are similar in the heterogeneity and no heterogeneity specifications. The UI variables always show strong effects in the directions found earlier. In two specifications, (4) and (5), the variance of the heterogeneity is significantly different from zero.³³ In several other specifications no heterogeneity is found. This result is puzzling; however, Monte Carlo experiments by Ridder and Verbakel (1983) show that a zero estimated variance is frequently found even when the estimated and true models are identical.

Specifications (5) and (7) allow a direct comparison of estimates with and without unobserved heterogeneity. The coefficients in the heterogeneity specification tend to be larger in absolute value as suggested by Lancaster, but among the UI coefficients only the benefit coefficient is appreciably larger. The spline in time until benefit exhaustion and the benefits expected to lapse variable are barely affected. The estimated baseline hazard becomes decidedly upward sloping however, when gamma heterogeneity is allowed. Compare the columns of Table VII to see this result.

Separate specifications analogous to (5) were estimated for the two subsamples discussed in Section 3. The estimates for the random sample are shown in specification (8). The estimates for the sample with mostly constant benefit lengths are shown in specification (9). In the random subsample, benefits have a smaller effect than in specification (5), but similar to specifications (6), (7), and (9). The effect of approaching the week benefits lapse is more pronounced, but the benefits expected to lapse effect is slightly smaller. The constant benefit length subsample also gives estimates similar to earlier ones, except the time until exhaustion spline is *U*-shaped as in Moffitt (1985).

6. SOME COMMENTS ON THE RESULTS AND CONCLUSIONS

In the preceding pages, all of the simpler specifications are rejected in favor of specification (5). For this reason, estimates from specification (5) are used to summarize the results. The coefficient on the UI benefit level is precisely estimated and implies that a 10 percent increase in benefits is associated with an 8.8 percent decrease in the hazard. The coefficient suggests a relatively large disincentive effect of UI. Somewhat smaller estimates are obtained when the same model is used on the random subsample in specification (8). The benefit coefficient implies that a 10 percent increase in benefits is associated with a 5.3 percent decrease in the hazard. Since specification (5) has a much larger heterogeneity variance, the two specifications may lead to similar expected duration elasticities. One should note that the estimates only apply to the

³³ The asymptotic *t* statistics for specifications (4) and (5) are 3.89 and 4.05, respectively. The critical values in a one-tailed test at the .05 and .01 levels are 1.64 and 2.33, respectively.

hazard prior to exhaustion. Higher benefits may lead to a higher hazard after exhaustion as suggested by Mortensen (1977).

The time until exhaustion spline coefficients are jointly highly significant; three of the coefficients are individually significantly different from zero at conventional levels. The point estimates imply that moving from 54 to 41 weeks until exhaustion raises the hazard by 46 percent. The hazard is essentially flat between 41 and 6 weeks, but the point estimates imply a small decrease in the hazard. Between 6 and 2 weeks before benefit exhaustion the hazard rises 109 percent. One week away the hazard rises an additional 95 percent. Cumulatively, the hazard more than quadruples as one moves from 6 weeks to 1 week until exhaustion. The pronounced rise in the hazard as exhaustion approaches supports the models of both Mortensen (1977) and Moffitt and Nicholson (1982).

While the spike in the hazard just before exhaustion is striking, few spells last sufficiently long to be affected by the spike. Almost all of the effect of UI on mean spell lengths comes from the level of benefits. Here the estimated effect of the benefit level is toward the high end of the distribution of recent estimates. A consensus of the previous estimates of the effect of a ten percentage point increase in the replacement ratio might be a one-half to one week increase in the length of spells.³⁴ Here the estimate is around one and one-half weeks. The results also differ from those of Topel (1983, 1984) who argues that the key mechanism by which UI raises the unemployment rate is the increased incidence of temporary layoffs rather than the lowered exit rate from unemployment.

Larger estimated effects are a plausible result of better data on spell length and the level and length of benefits. In the CPS data used by Topel, one must make an educated guess whether or not an individual is receiving benefits and then impute their level. Difficulties with the CPS length of unemployment spell variable were mentioned in Section 3. However, the estimates are sufficiently different from most other estimates that a further examination seems warranted. The sources of variation in benefit levels in the 12 state CWBH data are nonlinearities in the benefit schedules (especially different minima and maxima across states), legislative changes during the sample period, and the erosion of real benefit levels due to inflation between legislative changes. Differences in the average benefit generosity across states is absorbed by the state fixed effects in the specifications, and pre-UI weekly earnings are included as an explanatory variable.

³⁴ Hamermesh (1977) concludes that "the best estimate—if one chooses a single figure—is that a 10-percentage point increase in the gross replacement rate leads to an increase in the duration of insured unemployment of about half a week when labor markets are tight." Danziger, Haveman, and Plotnick (1981) report a wide range of estimates, but suggest that Moffitt and Nicholson (1982) offers the most reliable estimates. Their study found that a 10-percentage point increase in the replacement rate was associated with about a one week increase in the average length of unemployment spells. The estimates in Solon (1985) imply between a half a week and a full week increase in mean durations from a 10-percentage point increase in the replacement rate.

Within a given state at a point in time, the benefit level is usually a simple nonlinear function of previous earnings. Thus, it has been argued, most notably in Welch (1977), that effects of the benefit level on unemployment cannot be separated from the effects of previous earnings. While I believe it is extreme to apply this criticism to the data set used here, as a check on these estimates I am examining unemployment spell lengths in the months around changes in state UI laws.

*Department of Economics and Center for Urban Affairs and Policy Research,
Northwestern University, 2003 Sheridan Rd., Evanston, IL 60208, U.S.A.*

Manuscript received February, 1988; final revision received July, 1989.

REFERENCES

- ADAMS, J. (1986): "Equilibrium Taxation and Experience Rating in a Federal System of Unemployment Insurance," *Journal of Public Economics*, 29, 51-77.
- BAILEY, K. (1984): "Asymptotic Equivalence Between the Cox Estimator and the General Maximum Likelihood Estimator of Regression and Survival Parameters," *Annals of Statistics*, 12, 730-736.
- CHUNG, CHING-FAN, AND A. GOLDBERGER (1984): "Proportional Projections in Limited Dependent Variable Models," *Econometrica*, 52, 531-534.
- CORSON, WALTER, AND WALTER NICHOLSON (1983): "An Analysis of UI Recipients' Unemployment Spells," Unemployment Insurance Occasional Paper 83-1, U.S. Department of Labor, Employment and Training Administration.
- COX, D. R. (1972): "Regression Models and Life-Tables," *Journal of the Royal Statistical Society B*, 34, 187-202.
- (1975): "Partial Likelihood," *Biometrika*, 62, 269-276.
- DANZIGER, SHELDON, ROBERT HAVEMAN, AND ROBERT PLOTNICK (1981): "How Income Transfer Programs Affect Work, Savings, and the Income Distribution: A Critical Review," *Journal of Economic Literature*, 19, 975-1028.
- DEERE, DONALD, AND JEFFREY MIRON (1986): "The Cross Sectional Impact of Unemployment Insurance on Layoffs, Employment, and Wages," unpublished.
- FELDSTEIN, MARTIN (1975): "The Importance of Temporary Layoffs: An Empirical Analysis," *Brookings Papers on Economic Activity*, 5, 725-744.
- GREEN, JERRY, AND JOHN SHOVEN (1986): "The Effects of Interest Rates on Mortgage Prepayments," *Journal of Money, Credit, and Banking*, 18, 41-59.
- GREENE, WILLIAM H. (1981): "On the Asymptotic Bias of the Ordinary Least Squares Estimator of the Tobit Model," *Econometrica*, 49, 505-514.
- HAN, AARON, AND JERRY A. HAUSMAN (1986): "Semiparametric Estimation of Duration and Competing Risk Models," M.I.T. Working Paper.
- HAMERMESH, DANIEL S. (1977): *Jobless Pay and the Economy*. Baltimore: The Johns Hopkins University Press.
- HECKMAN, JAMES, AND BURTON SINGER (1984): "A Method for Minimizing the Distributional Assumptions in Econometric Models for Duration Data," *Econometrica*, 52, 271-320.
- HOUGAARD, PHILIP (1984): "Life Table Methods for Heterogeneous Populations: Distributions Describing the Heterogeneity," *Biometrika*, 71, 75-83.
- KATZ, LAWRENCE F. (1985): "Worker Mobility and Unemployment," Ph.D. Dissertation, MIT.
- (1986): "Layoffs, Recall and the Duration of Unemployment," NBER Working Paper No. 1825.
- KATZ, LAWRENCE F., AND BRUCE D. MEYER (1987): "Unemployment Insurance, Recall Expectations and Unemployment Outcomes," unpublished.
- (1988): "Unemployment Insurance, Recall Expectations and Unemployment Outcomes," NBER Working Paper No. 2594.

- KIEFER, J., AND J. WOLFOWITZ (1956): "Consistency of the Maximum Likelihood Estimator in the Presence of Infinitely Many Incidental Parameters," *Annals of Mathematical Statistics*, 27, 887-906.
- LANCASTER, TONY (1979): "Econometric Methods for the Duration of Unemployment," *Econometrica*, 47, 939-956.
- (1985): "Generalised Residuals and Heterogeneous Duration Models with Applications to the Weibull Model," *Journal of Econometrics*, 28, 155-169.
- LILIEN, DAVID (1980): "The Cyclical Importance of Temporary Layoffs," *The Review of Economics and Statistics*, 62, 24-31.
- MCCALL, BRIAN P. (1988): "Occupational Matching: A Test of Sorts," Working Paper #237, Industrial Relations Section, Princeton University.
- (1990): "Occupational Matching: A Test of Sorts," *Journal of Political Economy*, 98, 45-69.
- MEDOFF, JAMES (1979): "Layoffs and Alternatives under Trade Unions in U.S. Manufacturing," *The American Economic Review*, 69, 380-395.
- MEYER, BRUCE D. (1986): "Semiparametric Estimation of Hazard Models," Mimeo, MIT.
- MORTENSEN, DALE T. (1977): "Unemployment Insurance and Job Search Decisions," *Industrial and Labor Relations Review*, 30, 505-517.
- (1987): "A Structural Model of UI Benefit Effects on the Incidence and Duration of Unemployment," Mimeo, Northwestern University.
- MOFFITT, ROBERT (1985a): "Unemployment Insurance and the Distribution of Unemployment Spells," *Journal of Econometrics*, 28, 85-101.
- (1985b): "The Effect of the Duration of Unemployment Benefits on Work Incentives: An Analysis of Four Data Sets," Unemployment Insurance Occasional Paper 85-4, U.S. Department of Labor, Employment and Training Administration.
- MOFFITT, ROBERT, AND WALTER NICHOLSON (1982): "The Effect of Unemployment Insurance on Unemployment: The Case of Federal Supplemental Benefits," *The Review of Economics and Statistics*, 64, 1-11.
- POTERBA, JAMES, AND LAWRENCE SUMMERS (1984): "Survey Response Variation in the Current Population Survey," *Monthly Labor Review*, 107, 31-37.
- POWELL, JAMES L. (1984): "Least Absolute Deviations Estimation for the Censored Regression Model," *Journal of Econometrics*, 25, 303-325.
- PRENTICE, ROSS, AND L. GLOECKLER (1978): "Regression Analysis of Grouped Survival Data with Application to Breast Cancer Data," *Biometrics*, 34, 57-67.
- RIDDER, GEERT, AND WIM VERBAKEL (1983): "On the Estimation of the Proportional Hazard Model in the Presence of Unobserved Heterogeneity," mimeo, University of Amsterdam.
- SIDER, HAL (1985): "Unemployment Duration and Incidence: 1968-82," *American Economic Review*, 75, 465-472.
- SOLON, GARY (1985): "Work Incentive Effects of Taxing Unemployment Benefits," *Econometrica*, 53, 295-306.
- TOPEL, ROBERT H. (1983): "On Layoffs and Unemployment Insurance," *The American Economic Review*, 73, 541-559.
- (1984): "Equilibrium Earnings, Turnover, and Unemployment: New Evidence," *Journal of Labor Economics*, 2, 500-522.
- WELCH, FINIS (1977): "What Have We Learned from Empirical Studies of Unemployment Insurance?" *Industrial and Labor Relations Review*, 30, 451-461.

LINKED CITATIONS

- Page 1 of 7 -



You have printed the following article:

Unemployment Insurance and Unemployment Spells

Bruce D. Meyer

Econometrica, Vol. 58, No. 4. (Jul., 1990), pp. 757-782.

Stable URL:

<http://links.jstor.org/sici?sici=0012-9682%28199007%2958%3A4%3C757%3AUIAUS%3E2.0.CO%3B2-0>

This article references the following linked citations. If you are trying to access articles from an off-campus location, you may be required to first logon via your library web site to access JSTOR. Please visit your library's website or contact a librarian to learn about options for remote access to JSTOR.

[Footnotes]

² **What have We Learned from Empirical Studies of Unemployment Insurance?**

Finis Welch

Industrial and Labor Relations Review, Vol. 30, No. 4. (Jul., 1977), pp. 451-461.

Stable URL:

<http://links.jstor.org/sici?sici=0019-7939%28197707%2930%3A4%3C451%3AWHWLFE%3E2.0.CO%3B2-F>

² **How Income Transfer Programs Affect Work, Savings, and the Income Distribution: A Critical Review**

Sheldon Danziger; Robert Haveman; Robert Plotnick

Journal of Economic Literature, Vol. 19, No. 3. (Sep., 1981), pp. 975-1028.

Stable URL:

<http://links.jstor.org/sici?sici=0022-0515%28198109%2919%3A3%3C975%3AHITPAW%3E2.0.CO%3B2-I>

³ **Unemployment Insurance and Job Search Decisions**

Dale T. Mortensen

Industrial and Labor Relations Review, Vol. 30, No. 4. (Jul., 1977), pp. 505-517.

Stable URL:

<http://links.jstor.org/sici?sici=0019-7939%28197707%2930%3A4%3C505%3AUIAJSD%3E2.0.CO%3B2-5>

³ **Unemployment Insurance and Job Search Decisions**

Dale T. Mortensen

Industrial and Labor Relations Review, Vol. 30, No. 4. (Jul., 1977), pp. 505-517.

Stable URL:

<http://links.jstor.org/sici?sici=0019-7939%28197707%2930%3A4%3C505%3AUIAJSD%3E2.0.CO%3B2-5>

NOTE: *The reference numbering from the original has been maintained in this citation list.*

LINKED CITATIONS

- Page 2 of 7 -



¹⁶ **The Effects of Interest Rates on Mortgage Prepayments**

Jerry Green; John B. Shoven

Journal of Money, Credit and Banking, Vol. 18, No. 1. (Feb., 1986), pp. 41-59.

Stable URL:

<http://links.jstor.org/sici?sici=0022-2879%28198602%2918%3A1%3C41%3ATEOIRO%3E2.0.CO%3B2-X>

¹⁷ **Econometric Methods for the Duration of Unemployment**

Tony Lancaster

Econometrica, Vol. 47, No. 4. (Jul., 1979), pp. 939-956.

Stable URL:

<http://links.jstor.org/sici?sici=0012-9682%28197907%2947%3A4%3C939%3AEMFTDO%3E2.0.CO%3B2-7>

¹⁷ **A Method for Minimizing the Impact of Distributional Assumptions in Econometric Models for Duration Data**

J. Heckman; B. Singer

Econometrica, Vol. 52, No. 2. (Mar., 1984), pp. 271-320.

Stable URL:

<http://links.jstor.org/sici?sici=0012-9682%28198403%2952%3A2%3C271%3AAMFMTI%3E2.0.CO%3B2-U>

¹⁹ **Occupational Matching: A Test of Sorts**

Brian P. McCall

The Journal of Political Economy, Vol. 98, No. 1. (Feb., 1990), pp. 45-69.

Stable URL:

<http://links.jstor.org/sici?sici=0022-3808%28199002%2998%3A1%3C45%3AOMATOS%3E2.0.CO%3B2-N>

²⁰ **Life Table Methods for Heterogeneous Populations: Distributions Describing the Heterogeneity**

Philip Hougaard

Biometrika, Vol. 71, No. 1. (Apr., 1984), pp. 75-83.

Stable URL:

<http://links.jstor.org/sici?sici=0006-3444%28198404%2971%3A1%3C75%3ALTMFHP%3E2.0.CO%3B2-A>

³¹ **Econometric Methods for the Duration of Unemployment**

Tony Lancaster

Econometrica, Vol. 47, No. 4. (Jul., 1979), pp. 939-956.

Stable URL:

<http://links.jstor.org/sici?sici=0012-9682%28197907%2947%3A4%3C939%3AEMFTDO%3E2.0.CO%3B2-7>

NOTE: *The reference numbering from the original has been maintained in this citation list.*

LINKED CITATIONS

- Page 3 of 7 -



³² **The Importance of Temporary Layoffs: An Empirical Analysis**

Martin S. Feldstein

Brookings Papers on Economic Activity, Vol. 1975, No. 3. (1975), pp. 725-745.

Stable URL:

<http://links.jstor.org/sici?sici=0007-2303%281975%291975%3A3%3C725%3ATIOTLA%3E2.0.CO%3B2-C>

³² **The Cyclical Pattern of Temporary Layoffs in United States Manufacturing**

David M. Lilien

The Review of Economics and Statistics, Vol. 62, No. 1. (Feb., 1980), pp. 24-31.

Stable URL:

<http://links.jstor.org/sici?sici=0034-6535%28198002%2962%3A1%3C24%3ATCPOTL%3E2.0.CO%3B2-S>

³⁴ **How Income Transfer Programs Affect Work, Savings, and the Income Distribution: A Critical Review**

Sheldon Danziger; Robert Haveman; Robert Plotnick

Journal of Economic Literature, Vol. 19, No. 3. (Sep., 1981), pp. 975-1028.

Stable URL:

<http://links.jstor.org/sici?sici=0022-0515%28198109%2919%3A3%3C975%3AHITPAW%3E2.0.CO%3B2-I>

³⁴ **The Effect of Unemployment Insurance on Unemployment: The Case of Federal Supplemental Benefits**

Robert Moffitt; Walter Nicholson

The Review of Economics and Statistics, Vol. 64, No. 1. (Feb., 1982), pp. 1-11.

Stable URL:

<http://links.jstor.org/sici?sici=0034-6535%28198202%2964%3A1%3C1%3ATEOUIO%3E2.0.CO%3B2-0>

³⁴ **Work Incentive Effects of Taxing Unemployment Benefits**

Gary Solon

Econometrica, Vol. 53, No. 2. (Mar., 1985), pp. 295-306.

Stable URL:

<http://links.jstor.org/sici?sici=0012-9682%28198503%2953%3A2%3C295%3AWIEOTU%3E2.0.CO%3B2-2>

References

NOTE: *The reference numbering from the original has been maintained in this citation list.*

LINKED CITATIONS

- Page 4 of 7 -



Asymptotic Equivalence Between the Cox Estimator and the General ML Estimators of Regression and Survival Parameters in the Cox Model

Kent R. Bailey

The Annals of Statistics, Vol. 12, No. 2. (Jun., 1984), pp. 730-736.

Stable URL:

<http://links.jstor.org/sici?sici=0090-5364%28198406%2912%3A2%3C730%3AAEBTCE%3E2.0.CO%3B2-K>

Proportional Projections in Limited Dependent Variable Models

Ching-Fan Chung; Arthur S. Goldberger

Econometrica, Vol. 52, No. 2. (Mar., 1984), pp. 531-534.

Stable URL:

<http://links.jstor.org/sici?sici=0012-9682%28198403%2952%3A2%3C531%3APPILDV%3E2.0.CO%3B2-T>

How Income Transfer Programs Affect Work, Savings, and the Income Distribution: A Critical Review

Sheldon Danziger; Robert Haveman; Robert Plotnick

Journal of Economic Literature, Vol. 19, No. 3. (Sep., 1981), pp. 975-1028.

Stable URL:

<http://links.jstor.org/sici?sici=0022-0515%28198109%2919%3A3%3C975%3AHITPAW%3E2.0.CO%3B2-I>

The Importance of Temporary Layoffs: An Empirical Analysis

Martin S. Feldstein

Brookings Papers on Economic Activity, Vol. 1975, No. 3. (1975), pp. 725-745.

Stable URL:

<http://links.jstor.org/sici?sici=0007-2303%281975%291975%3A3%3C725%3ATIOTLA%3E2.0.CO%3B2-C>

The Effects of Interest Rates on Mortgage Prepayments

Jerry Green; John B. Shoven

Journal of Money, Credit and Banking, Vol. 18, No. 1. (Feb., 1986), pp. 41-59.

Stable URL:

<http://links.jstor.org/sici?sici=0022-2879%28198602%2918%3A1%3C41%3ATEOIRO%3E2.0.CO%3B2-X>

On the Asymptotic Bias of the Ordinary Least Squares Estimator of the Tobit Model

William H. Greene

Econometrica, Vol. 49, No. 2. (Mar., 1981), pp. 505-513.

Stable URL:

<http://links.jstor.org/sici?sici=0012-9682%28198103%2949%3A2%3C505%3AOTABOT%3E2.0.CO%3B2-O>

NOTE: *The reference numbering from the original has been maintained in this citation list.*

LINKED CITATIONS

- Page 5 of 7 -



A Method for Minimizing the Impact of Distributional Assumptions in Econometric Models for Duration Data

J. Heckman; B. Singer

Econometrica, Vol. 52, No. 2. (Mar., 1984), pp. 271-320.

Stable URL:

<http://links.jstor.org/sici?sici=0012-9682%28198403%2952%3A2%3C271%3AAMFMTI%3E2.0.CO%3B2-U>

Life Table Methods for Heterogeneous Populations: Distributions Describing the Heterogeneity

Philip Hougaard

Biometrika, Vol. 71, No. 1. (Apr., 1984), pp. 75-83.

Stable URL:

<http://links.jstor.org/sici?sici=0006-3444%28198404%2971%3A1%3C75%3ALTMFHP%3E2.0.CO%3B2-A>

Consistency of the Maximum Likelihood Estimator in the Presence of Infinitely Many Incidental Parameters

J. Kiefer; J. Wolfowitz

The Annals of Mathematical Statistics, Vol. 27, No. 4. (Dec., 1956), pp. 887-906.

Stable URL:

<http://links.jstor.org/sici?sici=0003-4851%28195612%2927%3A4%3C887%3ACOTMLE%3E2.0.CO%3B2-Z>

Econometric Methods for the Duration of Unemployment

Tony Lancaster

Econometrica, Vol. 47, No. 4. (Jul., 1979), pp. 939-956.

Stable URL:

<http://links.jstor.org/sici?sici=0012-9682%28197907%2947%3A4%3C939%3AEMFTDO%3E2.0.CO%3B2-7>

The Cyclical Pattern of Temporary Layoffs in United States Manufacturing

David M. Lilien

The Review of Economics and Statistics, Vol. 62, No. 1. (Feb., 1980), pp. 24-31.

Stable URL:

<http://links.jstor.org/sici?sici=0034-6535%28198002%2962%3A1%3C24%3ATCPOTL%3E2.0.CO%3B2-S>

Occupational Matching: A Test of Sorts

Brian P. McCall

The Journal of Political Economy, Vol. 98, No. 1. (Feb., 1990), pp. 45-69.

Stable URL:

<http://links.jstor.org/sici?sici=0022-3808%28199002%2998%3A1%3C45%3AOMATOS%3E2.0.CO%3B2-N>

NOTE: *The reference numbering from the original has been maintained in this citation list.*

LINKED CITATIONS

- Page 6 of 7 -



Layoffs and Alternatives under Trade Unions in U.S. Manufacturing

James L. Medoff

The American Economic Review, Vol. 69, No. 3. (Jun., 1979), pp. 380-395.

Stable URL:

<http://links.jstor.org/sici?sici=0002-8282%28197906%2969%3A3%3C380%3ALAAUTU%3E2.0.CO%3B2-8>

Unemployment Insurance and Job Search Decisions

Dale T. Mortensen

Industrial and Labor Relations Review, Vol. 30, No. 4. (Jul., 1977), pp. 505-517.

Stable URL:

<http://links.jstor.org/sici?sici=0019-7939%28197707%2930%3A4%3C505%3AUIAJSD%3E2.0.CO%3B2-5>

The Effect of Unemployment Insurance on Unemployment: The Case of Federal Supplemental Benefits

Robert Moffitt; Walter Nicholson

The Review of Economics and Statistics, Vol. 64, No. 1. (Feb., 1982), pp. 1-11.

Stable URL:

<http://links.jstor.org/sici?sici=0034-6535%28198202%2964%3A1%3C1%3ATEOUIO%3E2.0.CO%3B2-0>

Regression Analysis of Grouped Survival Data with Application to Breast Cancer Data

R. L. Prentice; L. A. Gloeckler

Biometrics, Vol. 34, No. 1. (Mar., 1978), pp. 57-67.

Stable URL:

<http://links.jstor.org/sici?sici=0006-341X%28197803%2934%3A1%3C57%3ARAOGSD%3E2.0.CO%3B2-X>

Unemployment Duration and Incidence: 1968-82

Hal Sider

The American Economic Review, Vol. 75, No. 3. (Jun., 1985), pp. 461-472.

Stable URL:

<http://links.jstor.org/sici?sici=0002-8282%28198506%2975%3A3%3C461%3AUDAI1%3E2.0.CO%3B2-1>

Work Incentive Effects of Taxing Unemployment Benefits

Gary Solon

Econometrica, Vol. 53, No. 2. (Mar., 1985), pp. 295-306.

Stable URL:

<http://links.jstor.org/sici?sici=0012-9682%28198503%2953%3A2%3C295%3AWIEOTU%3E2.0.CO%3B2-2>

NOTE: *The reference numbering from the original has been maintained in this citation list.*

LINKED CITATIONS

- Page 7 of 7 -



On Layoffs and Unemployment Insurance

Robert H. Topel

The American Economic Review, Vol. 73, No. 4. (Sep., 1983), pp. 541-559.

Stable URL:

<http://links.jstor.org/sici?sici=0002-8282%28198309%2973%3A4%3C541%3AOLAUI%3E2.0.CO%3B2-N>

Equilibrium Earnings, Turnover, and Unemployment: New Evidence

Robert H. Topel

Journal of Labor Economics, Vol. 2, No. 4. (Oct., 1984), pp. 500-522.

Stable URL:

<http://links.jstor.org/sici?sici=0734-306X%28198410%292%3A4%3C500%3AEETAUN%3E2.0.CO%3B2-5>

What have We Learned from Empirical Studies of Unemployment Insurance?

Finis Welch

Industrial and Labor Relations Review, Vol. 30, No. 4. (Jul., 1977), pp. 451-461.

Stable URL:

<http://links.jstor.org/sici?sici=0019-7939%28197707%2930%3A4%3C451%3AWHWLFE%3E2.0.CO%3B2-F>